

IV.—Some common Crinoid Names, and the Fixation of Nomenclature. By F. A. BATHER, M.A., D.Sc., F.R.S.

IN his paper "The Genus *Encrinus*" (Ann. & Mag. Nat. Hist. (8) iii. pp. 308–310, March 1909) Mr. Austin Hobart Clark opens with such pointed reference to my previous attempts at fixing the nomenclature of the crinoid genera involved that silence on my part might seem discourteous, or else to imply that I accepted all Mr. Clark's statements without demur. Since Mr. F. Springer has also taken up the question in an admirable paper ("A new American Jurassic Crinoid," Proc. U.S. Nat. Mus. xxxvi. pp. 170–190, pl. iv., 3rd March, 1909), I am at last persuaded to publish the following comments.

Mr. Clark makes the criticism that the name *Isocrinus* is not, as I said (1898), due to H. v. Meyer, but to L. Agassiz. He writes (1909, March, p. 308):—" *Isocrinus* was first proposed in 1836 (L. Agassiz, Mém. de Soc. de Sci. Nat. de Neuchâtel, i. p. 195, type *Isocrinites pendulus*, de (sic) Meyer, 1835, nomen nudum, = *Isocrinus pendulus*, von Meyer." I do not know whence Mr. Clark obtained either the spelling "*Isocrinites*" or the date "1835." In the British Museum copy of the memoir cited the words are "*I. pendulus* H. de M. (encore inédit)." Therefore, until v. Meyer (1837) published his description of *I. pendulus*, *Isocrinus* had no genotype, while its diagnosis was inadequate and incorrect: "Très-voisin des Pentacrines, dont il a la tige avec ses rayons simples. Les premiers articles des rayons du disque ne font pas saillie comme dans le genre *Pentacrinus*; en revanche, la partie supérieure de la tige est plus développée." But, apart from all this, Agassiz definitely assigned the name *Isocrinus* to "H. de Meyer," and was no doubt attempting to condense manuscript information supplied by that author. Had it not been for von Meyer's own paper (1837) the name *Isocrinus* would never have come up for discussion. How the generic name may be written and quoted by others is a matter of small importance. But the foregoing are the facts of the case.

Mr. Clark seems to hint at further ignorance on my part in reference to the names *Balanocrinus* and *Metacrinus*.

He says "*Balanocrinus* is not available for any genus of Pentacrinitidæ." It is admitted on p. 247 of my paper (1898) that "De Loriol has perhaps strained a point"; "but," I add, "nothing would be gained by contesting his action." The facts are these:—The name *Balanocrinus* occurs first in

Desor (1845, Bull. Soc. Sci. nat. Neuchâtel, i. p. 214) for "Pentacrinus ayant la face articulaire des anneaux de la tige crénelée sur son pourtour. Jusqu'ici on ne connaissait que des fragments de tiges de ce type. On en avait même distingué plusieurs espèces, les *Pentacrinus subteres* Münst. et *P. pentagonalis* Gldf.; mais on n'avait aucune idée des calices. M. Agassiz vient de découvrir, parmi les Crinoïdes du Musée de Bale, un calice en forme de gland, dont la base présente une articulation tout-à-fait semblable à celle du *Pentacrinus subteres*. Ce savant en a fait un genre à part, sous le nom de *Balanocrinus*, et il pense qu'on devra lui associer toutes les tiges qui présentent ce mode d'articulation." Is it not perfectly clear that, had the matter rested here, we should have been bound to adopt *Balanocrinus* Desor ex Agassiz MS., with genotype either *B. subteres* or *B. pentagonalis*? This, as it happens, is precisely what we all have done, the former species being taken as genotype. Why then does Mr. Clark say we are wrong? Because, as de Loriol has told us (1879, 'Crin. foss. Suisse,' pp. 163, 175, and 1888, 'Paléont. franç. Crin. jurass.' p. 295), the "calice en forme de gland" proved to be nothing but a stem-fragment of *Millericrinus* (? *M. mattheyi*), swollen owing to the attacks of a parasite. But this fragment, being neither described nor named by Agassiz, afforded no species to serve as genotype. It is true that the supposed discovery of a calyx led Agassiz to found his genus; but this quotation from Desor shows that the diagnostic character was derived from the joint-face of the stem. Clearly Agassiz thought he was dealing with a *Pentacrinus subteres*, and that species, if any, would have been his genotype. To try to avoid the natural conclusions from these undisputed facts requires more than legal subtlety and brings no advantage to anybody.

"*Metacrinus*," says Mr. Clark quite correctly, "was first diagnosed in 1882." I did not in 1898 give any other date, or any date at all, since I was not discussing *Metacrinus*. I did, however, take from the paper to which he refers (P. H. Carpenter, 1882, Bull. Mus. Comp. Zool. Harvard, x. p. 167) a statement as to the origin of the name. It may, nevertheless, be pointed out that Carpenter's reference to *Metacrinus* in that paper (1882) was rather in the nature of a passing allusion to Wyville Thomson's MS. name, and that no species was then described or even mentioned by name. Therefore in the Echinoderma volume of the 'Treatise on Zoology' (1900) it seemed more useful to refer the student to the complete description in the 'Challenger' Report of 1884.

Mr. Clark's statements concerning the name *Encrinus* involve more serious questions. Passing over various writers after 1758, he stops at Blumenbach (1779, 'Handb. d. Naturgeschichte'), and finds that the name must be applied to the ordinary *Pentacrinus asteria*, which is now generally called *Isocrinus* by writers on crinoids. This may be the correct inference, but it seems hard that the absurdity should have to be fathered on Blumenbach. That eminent naturalist professed to be writing a manual for the elementary student and the amateur rather than a complete systematic treatise, and in dealing with the recent Echinoderma, or Cartilaginea as he called them, he used only the commonly known names, *Echinus*, *Asterias*, and *Encrinus*. Had he been asked why he referred the *Isis asteria* of Linnæus to *Encrinus*, he would doubtless have replied in the words of John Ellis (1762, Phil. Trans. lii. p. 358), "As it comes nearest to the fossils called encrini, or lili lapidei [*sic*], I shall keep that name, and call it *Encrinus*, etc." But in the second part of the same work, in the Abschnitt 'Von den Versteinerungen,' we find "Die Encriniten und Pentacriniten" quite clearly distinguished, and it was with the latter alone that Blumenbach compared his *Encrinus asteria*. A few years later (1790, Voigt's Mag. f. d. neueste a. d. Physik, vi. Heft 4, pp. 1-17) he was severe on Hollmann for having confused *Pentacrinus* with *Encrinus*. Since the days of Lachmund (1669) the name *Encrinus* had been in constant use for the *Lilium lapideum* of the Muschelkalk, and it cannot be supposed that Blumenbach had the smallest intention of diverting it from this well-known use. To preserve this older meaning, however, we are compelled by the modern rules of nomenclature to find some instance of its application before 1779 and after 1758.

Mr. Springer (1909) and Mr. A. H. Clark (1908, Proc. U.S. Nat. Mus. xxxiv. p. 517) both refer to *Encrinus coraloides* Andreae (1763), but both have had the misfortune to quote Andreae incorrectly though diversely. The figures actually referred to by Andreae represent stem-fragments that cannot, in my opinion, be referred with certainty to any species or genus. If this is to be the basis of *Encrinus*, the name will simply disappear from actual use. To rescue it, something earlier and more intelligible must be sought for.

The desired application seemed to have been found in C. F. Schulze (1760, 'Betrachtung d. versteinerten See-sterne'); but Mr. Clark asks why I should take this and not take Schulze's *Decacnimos*, *Polyactinis*, and *Triscædecacnimos* instead of the later *Antedon* and *Actinometra*. The simplest answer to this is that, whereas I have had occasion to go fully

into the history of *Encrinus*, I have made no serious attempt to deal with *Antedon* and *Actinometra*. Had I proceeded to the task of subdividing those genera—a task which I foreshadowed in the 'Annals' in 1891, but which has now been accomplished by Mr. Clark—then I should have attempted to revise the nomenclature. So far as possible I leave names alone until the need for publishing new facts involves more precise definition or discrimination.

Another reason is that in the case of *Encrinus* there is no possible room for doubt as to Schulze's meaning, because he gives excellent figures of the fossil usually known as *Encrinus liliiformis*. In the case of the other names few would be bold enough to say definitely to which species each of them refers.

But perhaps the truest reason is that I have long been aware of the facts recounted in Mr. Clark's paper and of others given in the more complete history just published by Mr. F. Springer (1909), and I saw that terrible difficulties would arise if Schulze's *Encrinus* were not accepted. Overwhelmed by the thought, I clutched at the first obvious straw, letting the rest of the bundle drift whither it would.

Of course I am prepared to accept the contention of those who have recently examined the work of Schulze (viz. A. H. Clark, 1908, "The Nomenclature of the Recent Crinoids," Proc. U.S. National Mus. xxxiv. 435-542; W. K. Fisher, 1908, "Necessary Changes in the Nomenclature of Starfishes," Smithsonian. Miscell. Coll. Quart. lii. 87-93; F. Springer, 1909, *op. cit.*; and others), and to admit that his names are not always binomial. Some are, but others are not. Binomial nomenclature was in the air, and to writers after 1758 I have generally given the benefit of the doubt. I did not pretend that Schulze used *Encrinus* with any trivial name attached, but I took the generic name alone, and as there was never any doubt to what it referred, it still seems to me as well established as, say, *Isocrinus* Agassiz, 1836, or *Metacrinus* Carpenter, 1882, both of them introduced in a similar manner (*i. e.* the names quoted from others), but without any described or figured species by which their far less complete or even less correct diagnoses could be interpreted. Except on the purely pedantic and arbitrary criterion of a consistent use of binomial nomenclature, *Encrinus* Schulze certainly has the advantage.

But, after all, nothing will ultimately be gained by blinking facts or seeking to escape from rules. Suppose we give up Schulze and face the consequences, of which Mr. Clark only shows us a few, but which, as Mr. Springer points out, are

many and disastrous whatever other solution we attempt. Then, much as I admire the learned argument and legal skill with which Mr. Springer invokes the doctrine of prescription, I consider that there is a danger in the introduction of such a principle as lapse of time. Who is to decide what period shall be set? And in such a case as the present it might be urged that the use of *Encrinus* from Blumenbach to Lamarek should be weighed against the subsequent use from the days of J. S. Miller. When once rational argument is admitted to such a dispute the controversy may go on for ever. The simplest solution of the gordian knot was given once for all by Alexander. Let us dare on occasion to be no less arbitrary. But it will never do for each to act according to his own idea of what is "common sense," if only because sense in these matters never is common to all. The only possible alternative to strict following of rules is that zoologists should agree to accept as final the decision of some authority by them appointed. The vehicle for such authority already exists in the Nomenclature Committee of the International Zoological Congress, the only body that has any claim to represent either all branches of zoology or all nationalities.

If I may indicate a convenient form of procedure, I would suggest that those zoologists who wish to protect certain names should lay the complete facts of the case before the Committee, and should accompany their request for the retention of certain definite names in defiance of the Rules by the signatures of as many workers on the group affected as they can obtain. Due announcement of the proposed step should be made in certain widely circulated journals, and a reasonable time should be allowed for the reception of protests. The Committee should ultimately give its decision, and this decision should be published in the aforesaid journals. A summary of the labours of the Committee in this direction would of course be given from time to time in the publications of the International Zoological Congress.

Some of my zoological colleagues appear to mistrust the Nomenclature Committee of the International Congress. Should their opinion be widely shared, it might prove that zoologists at large would not agree beforehand to submit to the ruling of that Committee. As an alternative body, the International Congress of Academies may be suggested. This at present does not appear to number among its representatives many zoologists familiar with the problems of nomenclature, but it could no doubt appoint a committee with the necessary qualifications.

The precise style or mode of appointment of the desired authority does not greatly matter if only zoologists will agree to accept it. But that it should consist of experts will doubtless be conceded. The ruling may be arbitrary, but it must none the less be made with knowledge of all the circumstances of the case and of the results that will follow from it. It must be clearly understood that the decision is to be made, not because it is in accordance with the rules, but because it is to produce practical convenience.

There is nothing particularly novel in these proposals. A similar one was made in 'Natural Science' for April and May 1896 (pp. 218-220, 302), but though "regarded with favour in various influential quarters," nothing has yet been done to give it effect. And even the recent discussion at the British Association, though unanimous in its resolutions, has so far been barren in its results. The next steps appear to be, first to find out whether a sufficient number of leading zoologists are in favour of these proposals, the next to approach whichever of the two bodies mentioned may be agreed upon, with a request that it will undertake this added responsibility. This would be better done by some society or some group of naturalists than by a single worker known only to a few. Perhaps the British Association would appoint a small committee to collect opinions and formulate the request.

P.S.—To prevent misconception, it may be added that this paper was written before the receipt of Mr. Springer's widely distributed appeal. He, however, deals only with a particular question, capable, as I have here shown, of various answers. My object is to press for a solution of the general question.

V.—Notes on *Merlia normani*, Kirkp.

By R. KIRKPATRICK.

PROF. WELTNER, to whom I had sent, at his request, some specimens of *Merlia normani* which I had dredged up off Porto Santo Island, has recently published a notice* entitled "Ist *Merlia normani* Kirkp. ein Schwamm?"

The pressure of other work prevents me from giving here a full description of *Merlia* adequately illustrated; nor am I yet able to answer the question "What is *Merlia*?"—this inability partly being due, I think I may fairly say, to the

* Archiv für Naturg. 75 Jahrg. 1 Bd., 1 Heft, 1909, p. 139.